

plague, diseases rarely seen in this country nowadays, we think that brief accounts of Mediterranean fever and cholera might have been included with advantage.

The book is profusely illustrated, and some of the photographs, though only in black and white, give a remarkably good idea of the characters and distribution of rashes.

R. T. H.

Beschrijving en Onderzoek van den gyroscopischen Horizon Fleuriais (Model Ponthus et Therrode).

By L. Roosenburg. Pp. 94; 3 plates. (Utrecht: Kemink & Zoon, 1909.)

IN this pamphlet the author describes some improvements introduced into the form of gyrostatic horizon proposed by Fleuriais. In the original construction, a top rotated in a chamber from which the air had been removed, and the whole could be fixed to a sextant in front of the horizon glass. Upon the top was placed a glass scale, with arrangements for reflecting the divisions of the scale into the sextant telescope in a direction parallel to the equator of the top. The angle subtended by the divisions of the scale was ten minutes, and the position of the object was estimated on this scale.

In the new form here described, a temporary vacuum only is made, and the chamber can be opened for the inspection of the parts, and renewal of the top point and the cup in which it rotates. The top is set in motion by an air-pump, which also creates the vacuum. Observations are possible for fifteen or twenty minutes. After the top has been rotating some six or seven minutes and the precessional effects rendered negligible, the sextant is clamped with the sun or star in the field of view, and a considerable number of readings taken of the position of the object on the scale. Lastly, the reading of the sextant is taken.

The author insists on the necessity of a large number of readings in order to get good results, apparently to eliminate the effect of irregular motion, which in unfavourable circumstances can amount to 13' in three seconds. It is contended that though practice with the instrument is necessary, it is not difficult to use, is, in fact, easily mastered, and is equally available for stars as the sun. The results of more than 200 observations are given, and, with a few exceptions, the errors of altitude are always less than 3'. The author concludes that it is a trustworthy and very serviceable instrument for the determination of position at sea, preferable to other forms of the same class.

Revue de Géographie annuelle. Publiée sous la Direction de M. Ch. Vélain. Tome ii., Année 1908. Pp. 730. (Paris: Ch. Delagrave.) Price 15 francs.

THIS volume of the "Revue" ranges no less widely than the preceding one. As regional geography we find classified "Étude analytique du Relief de la Corse," by J. Deprat, and "Le Pérou," by C. Guibeaud. In the mathematical department G. Perrier deals with the figure of the earth and important geodetic operations, and A. Berget writes on "Les Méthodes et les Instruments du Géographe Voyageur." M. Zimmerman provides a review of half a century of European colonisation, and P. Girardin studies the subject of glaciation in the most recent geological epoch.

The first of these papers, that on Corsica, is an important contribution to the geography and geology of an island which has not been as closely studied as might be supposed from its accessibility. M. Perrier deals principally with the new measurement of the arc of the meridian of Quito which is in the hands of the Service géographique de l'Armée. It has been

in progress for nearly ten years, of which the field work alone occupied five, and its results are far from complete as yet.

The article on Peru by M. Guibeaud is a general geographical study, most useful in its way. First it provides a short survey of the country according to natural regions, and then passes on to a consideration of its chief economic, ethnographical, and political aspects. This article is particularly well illustrated. M. Zimmerman's study of colonisation is a careful collection of facts and theories, with copious references to authorities, which should form an excellent foundation for the investigation of this subject of world-importance. The volume, judged on French standards, is particularly well printed and produced. It is heavy and bulks large, and not a few readers would no doubt like to be able to obtain one or other of its component articles separately.

Notes on Dynamics. By Sir G. Greenhill. Second Edition. Pp. 221. (London: His Majesty's Stationery Office, 1909.) Price 3s.

THIS cheap issue from His Majesty's Stationery Office of a second edition of Sir George Greenhill's notes, prepared for the advanced class of the Ordnance College, Woolwich, will, we hope, become known to teachers and students. The title is modest, the book has never been advertised, and few people are aware of its great value and originality. It contains many excellent numerical examples, rather different from those which teachers usually set in elementary dynamics classes, but the reader will be even more interested in letting the author carry him occasionally into problems which are quite outside any ordinary curriculum. When he deals with problems which are dealt with in the text-books, he takes a way of his own in each case, and gives us new ideas. The end sections dealing with the stability of rigid bodies moving in fluids are of great interest.

J. P.

LETTERS TO THE EDITOR.

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The Temperature of the Upper Atmosphere.

LIKE Dr. Chree (NATURE, June 3), I do not think the term "stratosphere" a suitable one, and isothermal layer is obviously open to criticism. We should all be indebted to Dr. Chree if he would suggest a better and more accurate term. Isothermal column appears to me sufficiently accurate to describe the phenomena over a single station, but cannot be applied to the whole upper part of the atmosphere. Some single word implying the absence of vertical circulation is required.

There is no reasonable doubt that the daily temperature variation becomes insignificant at a height of 1 km., and hence one is apt to infer that it is negligible at 10 km.; but the observations are not sufficiently well distributed, and in my opinion the effect of solar radiation on the balloon, if not on the instruments, is too uncertain for us to speak positively about a daily variation at such heights. The two years' observations in England have shown no annual temperature variation above 10 km., and I do not believe that there is any definite change from summer to winter. It is probable that the mean monthly temperatures at 10 km. do not differ greatly either with latitude or with the season, although all the observations yet available at 15 km. show lower temperatures over the tropics than over temperate latitudes at that greater height.

I am glad to see that Dr. Chree does not assert that errors of $\pm 10^\circ$ F. are the usual thing. Probably few of those who use the instruments would assert that such an error might not occasionally occur. If the figures for

November 11, 1907, quoted by Dr. Chree, stood alone, one would readily accept his explanation, but since that date many similar differences, though not quite so large, have been recorded. In fact, the noticeable point about the so-called isothermal layer is the very large differences of temperature that are found at the same time over places a few hundred miles apart, and over the same place within a period of twenty-four hours. Because we cannot explain the phenomena, are we, therefore, to doubt their existence? It is perfectly natural to do so; the question is simply one of the credibility of the evidence.

The evidence is of various kinds. If one of the instruments used in England be completely immersed in a bath of liquid by an observer A, the temperature of the bath being, say, between $+30^{\circ}$ C. and -50° C., a second observer B having the record and the instrument can ascertain within 1° C., or at the most 2° C., the temperature of the bath used by A. Why, then, cannot B equally well ascertain the temperature of the air through which the balloon has carried the instrument? Secondly, these instruments are carried up by a balloon travelling through air that has been in contact with the balloon; the balloon in general bursts, and they fall, moving now at a much greater speed, since in England we use no parachute. Two traces are made, the one showing the temperature during the ascent, the other during the descent, but it is not often possible to say which is which. As a rule, the two traces are quite distinct; mostly, one indicates a temperature of from 1° C. to 3° C. below the other throughout, but sometimes the traces cross and re-cross each other. However, the point is that the two traces are practically identical; any peculiarity of gradient shown on the one is reproduced at the same height on the other. Now I think it lies with those who imply that our instrumental records are untrustworthy to explain this. If the temperatures shown by these two traces are not the approximate temperatures of the air, what are they? Systematic errors could not be the same in the different circumstances of the ascent and descent. It is inconceivable that casual errors could always so combine as to give errors of the same magnitude in pairs time after time. It is even less likely than that a man, drawing coloured balls from a bag, should draw the same colour in every two consecutive draws, for not only is the general trace reproduced, but every peculiarity in it is also reproduced.

Thirdly, the results obtained on the Continent and in America agree perfectly with those obtained with different instruments and a different system in England. This alone is not a good argument against the possibility of large casual errors, since casual errors are eliminated in the means, but the two sets of observations are as yet not very numerous—about 100 in England—and they show the same general relation between the temperature and height of the isothermal column and the height of the barometer at the surface.

Dr. Chree, from the last paragraph of his letter, appears to think that the instrument makers supply the scale. This is not the case in England, and I do not think it is abroad. Almost every instrument sent up in England to the present time has been made here. The University of Manchester is responsible for the scales of those that it sends up, and I am responsible for the scales of the rest. These scales are verified before and after each ascent. The lag in our instruments is very small, since we depend on the expansion and contraction of a strip of very thin German silver, but I do not see that the lag affects the general question, since it will be largely eliminated if we take the mean of the ascent and descent.

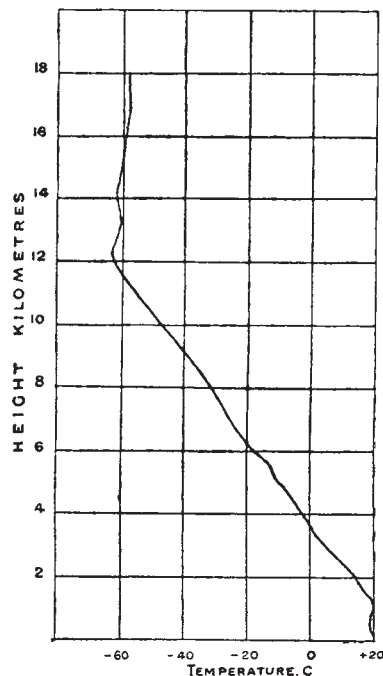
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As one who subscribed to the "Confession of Monaco," may I be allowed to say that no definitions of the names stratosphere and isothermal layer were supplied at the conference as those present understood the terms? The meaning of a word has often divided the orthodox from the heterodox, and for the benefit of Dr. Chree, and also of "heretics in England," I will endeavour to make the matter clearer. Balloon ascents show that, apart from irregularities near the surface, the temperature of the air

decreases with height fairly regularly up to a certain point; above this point the regular decrease ceases, and for still greater heights the temperature changes are very small; sometimes there is a small increase, sometimes a small decrease, and sometimes the temperature remains almost constant up to the greatest height reached by the balloon. At any one place and time it thus appears that the atmosphere is divided into two layers, which differ markedly from one another in their vertical temperature distributions.

A diagram from an actual ascent made here on October 1 of last year shows the two characteristic temperature gradients. To the upper layer the names isothermal layer and stratosphere have been given; the latter name is due to M. Teisserenc de Bort, who surmises that the lower layer, or troposphere, is the part of the atmosphere concerned in the vertical circulation associated with cyclones and anticyclones, while the stratosphere lies above such movements. The name isothermal layer is not a fortunate one; certainly none of the orthodox who were assembled at Monaco would maintain that the upper layer is isothermal either in time or in a horizontal direction. Some less misleading term might have increased the number of the "elect." Both terms, however, are now in general use, and give definite names to a definite thing, which, as Huxley said, is the object of nomenclature.



The characteristic temperature gradient of the upper layer has been found over all parts of Europe, over the Atlantic, and over North America, but near the equator, if it exists at all, it is at a much higher altitude than in temperate latitudes. Its absence over the equator, and the fact that lower temperatures have been recorded there than in any other part of the atmosphere, seems to me to be a further proof, if such were needed, that the temperature gradient of the upper air recorded in other places is not the result of instrumental error.

CHARLES J. P. CAVE.

Ditcham Park, Petersfield, June 6.

The Sense of Proximity.

IN NATURE for March 11 there is an interesting account by Dr. McKendrick of some investigations by Kunz, of Mülhausen, and Prof. Griesbach, on the senses of the blind. Among other points that he refers to and discusses is the question of the ability of the blind to avoid obstacles and find their way about. This calls to my mind some observations and experiments which I made upon myself some eleven years ago with reference to my ability to find my way about with my eyes shut or in the dark. These I had intended to extend and amplify, but up to the present these further experiments have been crowded out by press of other work.

Many people have the feeling that if, for instance, they are in a room in the dark, they have some perception of their relation to objects in the room, and particularly can appreciate when they are near one of the walls. I can remember having had this feeling for many years, but never had the opportunity of putting it to scientific test